LETTERS TO THE EDITOR.

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Vibrissæ on the Forepaws of Mammals.

IT is well enough known that carnivorous and other—especially nocturnal—animals are provided with numerous long hairs, generally called vibrissæ, upon various regions of the face. The "whiskers" of the cat are a familiar example. But it is not so widely known that there exists very commonly in those same creatures a tuft of long hairs upon the wrist, which are connected with a large nerve. There have been incidental references to these structures; thus Mr. Bland Sutton described and figured them in several Lemurs. But it is not, I believe, a matter of common knowledge that they are present in a great variety of mammals. I have examined members of the groups, Lemuroidea, Carnivora, Rodents, and Marsupials, and invariably found these structures in those members of the groups in question which use their forepaws as climbing or grasping organs, or in both ways. They are generally not very conspicuous, as the individual hairs are often not markedly thicker than those of the surrounding fur. But often they contrast by their colour. In a pale, almost albino, example of the squirrel *Sciurus maximus*, the hairs were especially obvious, owing to their being black, and thus contrasting with the pale brown of the surrounding part of the pelage. In a black cat the same vibrissæ were white. It is always, however, easy to assure oneself of their presence by the sense of touch. The bundle of these rather stiff hairs and the thick nerve termination cannot be missed, if the skin be gently squeezed. In a newly born phalanger this structure was particularly obvious; but in a kangaroo of corresponding age there were no signs of an elevation of the skin bearing thick hairs. It will be remembered that the mode of life of these two marsupials is very different. Although I have examined up to the present but few genera of mammals, it appears to me that this structure will be found to be pretty universal. I have of course not detected these arm vibrissæ in Ungulates.

Zoological Society's Gardens. FRANK E. BEDDARD.

The Distance to which the Firing of Heavy Guns is Heard.

IN the number of NATURE for August 16, there is an article by Mr. Charles Davison on the distance to which the firing of heavy guns is heard. The writer of the article seems to wish to collect facts bearing upon this question. I can supply one bit of information of the kind desired.

In the summer of 1863, during the siege of Charleston, S.C., by the Federal forces, being at the time an officer in the Confederate Army, I went, under orders, from Macon, Ga., to Charleston by way of Millen, Augusta and Branchville. It was just at the time of the first heavy naval bombardment of Port Sumtra. The train stopping at a water tank a few miles (I do not now remember just how many) on the Macon side of Millen, and therefore somewhat farther than this place from Charleston, I heard distinctly, not only the general, more or less varying, roar of the bombardment, but also the low boom of individual guns. The sound was faint, but unmistakable in the stillness while the engine was taking water, but was lost as soon as the train got into motion again and its noise began. At Augusta, during the stop made there, I could catch the sound of the guns again, though it was interfered with a good deal by the confused noise of a large town. At Branchville, a hamlet of a few houses, the sound was easily recognised by any one, and was accompanied by a general *feeling* of tremor.

Millen is nearly due west from Charleston, and distant about 117 miles in a direct line. Augusta is approximately 25° north of west from Charleston, and about 122 miles distant. Branchville is about 35° north of west from Charleston, and at a distance of about sixty miles.

Mr. Davison says that he has but little information as to the distance at which the discharge of *single guns* has been heard. I may therefore add that the heaviest guns in use in the bombardment I refer to were the 15-inch smooth bore muzzle-loading guns carried by the Federal turreted "Monitors." I do not remember now what was reported to be the charge of powder used, but they were, of course, firing shotted cartridges—some solid shot, but more frequently shell. J. W. MALLET.

The Solidification of Alloys.

IN a recent discussion on alloys, which took place at the Bradford Meeting of the British Association in the Section of Chemistry, a curious uncertainty was alluded to, which occurs in the cooling of certain alloys from the liquid state, as to the relative proportions of different varieties of crystals which form, depending on the rate that the cooling is proceeding with.

I would wish to draw the attention of those more particularly interested in the matter to a direction in which to look for what may be one of the causes of this peculiarity, namely, to the effect that different conductivities for heat in the different kinds of crystals may exercise in determining the relative proportions in which they form, where, as in this case, two or more varieties are possible. Where there is a difference in the conductivity of two possible varieties, the more of the better conducting material that is formed the faster in general the cooling can proceed.

The matter might be looked upon as a kind of inorganic evolution. Suppose that in the first instance round the boundaries, through which heat is passing out, of the cooling material, the two varieties form with equal facility, where the better conducting material forms heat escapes fastest and solidification of the molten material proceeds fastest, we may suppose this to follow in composition the lines of the crystals in proximity, namely, of the better conducting kind. Thus, by a kind of survival of the fittest, one of the varieties prevails.

When the cooling is very slow, where in the limit the temperature is at any moment the same throughout, this controlling influence is a vanishing quantity.

A similar principle is probably the cause of the radiating structure seen often in a cooled mass of certain materials, such as bismuth and possibly ice, which have different conductivities in different directions in the crystal. FRED. T. TROUTON.

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The Reform of Mathematical Teaching.

As I am in full sympathy with Prof. Perry's views, my own training, somewhat on the lines suggested by him, may be of interest. I was once taught Euclid and thoroughly hated the subject. At thirteen I was sent to school in Germany, where I was taught geometry; it had so little resemblance to Euclid that I looked on it as a new subject and was delighted with it. After eighteen months I returned to England to serve my apprenticeship, but not before I had advanced as far as solid geometry, quadratic equations and trigonometry, and I believe that this early and rapid mathematical training was of inestimable advantage to me in the works. It seems unconsciously to have led me to look on practical subjects with so much of a mathematical feeling that even now my fellow engineers consider me very mathematical, yet all the subsequent mathematical training at college (Germany) only extended over another eighteen months, and I admit that I would have liked to have had more.

I now come in contact with many engineers, both old and young, and almost invariably find that they are unmathematical, *i.e.*, they cannot look at an engineering problem with an analytical eye; and no wonder, if they have been brought up on Euclid. To me these volumes seem to be a collection of mathematical puzzles, which the ancient Greeks sent each other for solution, and which are most excellently edited by Euclid. A similar collection might nowadays be made of the trying problems in the chess columns of our daily literature, and these might be so pieced together as to afford most excellent mental training, but such a work would never teach good chess playing. It would be an excellent reference book for past masters, and that is what Euclid would still be if higher mathematics had not been invented. C. E. STROMEYER.

Lancefield, West Didsbury.

Leaf Decay and Autumn Tints.

"OBSERVATION shows," says Emile Mer, "that in most cases where wood dies in contact with living wood there is produced from the second towards the first a migration of starch and tannin, and (in a conifer) of resin; there is thus produced from the portions remaining living towards the dying or dead portions a drainage of substances, &c." These remarks refer to the formation of secondary periderm and of the duramen, but their scope and tenour may perhaps, I think, be extended to the case of forest leaves approaching the end of their existence as living organs. How does it come to pass that in autumn the leaves of some of our forest trees exhibit a brilliant livery of crimson, while others exhibit only a yellow or golden glory? Take, for instance, the case of the ash constituents in the dry substance of the leaf. It is known by analysis that the percentage of ash increases through nearly the whole life of the leaf in beech, sycamore, elm, but not in oak, larch, cherry, &c.; it depends a good deal on whether some one ash constituent (generally lime or silica) is being steadily stored up. For example, the dry leaf of *Acer campestre* on May 1 has 6 per cent. ash, and in October 16·2 per cent. ash; the dry leaf of *Prunus avium* has on April 28, 7·8 per cent., and on October 2, 7·2 per cent. ash. Now the leaf of the former tree is only yellow in autumn and never red, while that of the latter is very often beautifully crimson. In the former case there is a kind of gradual decay or death of some of the cells (mostly of the upper epidermis) which occasions a drainage of mineral and organic substances to these parts from the still living tissues. This drainage and accumulation attest, in fact, such a decay; and what is more, they seem to have a distinct influence over the ultimate autumnal coloration of the leaf itself. It is easy to understand, in fact, that the leaves which exhibit such a decay and approach to dissolution are just these wherein the chromogen precursive of the brilliant red coloration would likewise suffer an analogous kind of change, *i.e.* it would tend to become brown, to produce phlobaphene, just as it does in the outer bark which is the practically dead portion of the rind. Where this accumulation of mineral matter and all which it implies does not take place, as in cherries, currants, American oaks, pears, wild vine, barberry, &c., then the chromogen does not deteriorate; it evolves its proper pigment, and assumes the flush and glow of active living colour. On the other hand, in elms, chestnut, linden, birch, poplars, &c., which are never red but only yellow, it is only the vivid carotin attached to the last faded and now exhausted chlorophyll which gleams forth, but only for a time, and if not too much obstructed by the dull browns of decomposed carbohydrates and superoxidised tannic chromogens.

P. Q. KEEGAN.

Patterdale, Westmorland.

Homochronous Heredity and Changes of Pronunciation.

SEEING that in ancient German, or rather Gothic, Swedish, Danish, probably in French, and possibly in Sardinian, the *th* sound surviving in English (though much less frequent than it used to be) was once largely used, but nowadays Frenchmen and Germans find a difficulty with it, I should like to know whether systematic experiments have been made as to whether children of various ages of these two nationalities can pronounce it more exactly and spontaneously than their compatriots of a maturer age? I should like to make the same inquiry concerning English children and their pronunciation of the gutturals discarded or altered in such words as *night*, *bough* or *laugh*?

CHARLES G. STUART-MENTEATH.

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Authorities:—Helfenstein, "Comparative Grammar of the Teutonic Languages," 1870, pp. 156–9; G. Koerting, "Neugriechisch und Romanisch," 1896, p. 23; W. Meyer-Luebke, "Grammatik der Romanischen Sprachen," 1890, p. 428.

The Daylight Meteor of Sunday, September 2.

As Mr. Denning expresses a wish in your issue of September 13 for further information concerning this meteor, I write to inform you of what I saw myself.

I observed the time at which the meteor fell, and made it 6.50, but my watch is no chronometer. I saw the meteor from the road, between Deganwy and Llandudno, and it appeared to fall over the Little Orme's head. If you join this point to Leyburn in Yorkshire, you have the line as near as I can give it, and I do not think it is very far out. I did not note any column of smoke or cloud after the meteor fell. Its path was vertical. Some one says its angle of appearance was 35°, and disappearance 25°, which I should say is about correct. The sun was shining brightly, though low down in the west. The brilliance was greatest just before disappearance. I have never before seen any meteor to compare with it in brightness.

38 Hillfield Road, Hampstead. T. ROOKE.

THE meteor of September 2, described in your issue of September 13, was seen in Ireland also, in even brighter daylight.

I noted the time, 6.27 p.m. (Irish), and the direction, E.N.E., from a point near Enniskerry, co. Wicklow. There was a possible error of a couple of minutes in my watch, and a considerable error possible in the estimated direction, which was a rough approximation made without a compass.

B. ST. G. LEFROY.

THE THEORY OF IONS.

EVER since Faraday enunciated the law of electrolysis, that the same quantity of electricity passed when chemically equivalent masses of different substances were produced, it has been a matter of speculation whether this may not be due to atomic charges of electricity. Every one, in describing electrolysis and explaining how the substances evolved appeared at the electrodes without any apparent action in between them, based his description and explanation upon the supposition of electric charges on the atoms. Some substances, such as hydrogen, were given positive, and some, such as chlorine, were given negative charges, and the electric current through the liquid was explained as due to the convection of these charges by the moving atoms or groups of atoms, and the movements of these were ascribed to the electric force acting on these charges. The amount of the charge on each atom or group of atoms was proportional to its valency, and as this has with good reason always been taken as a whole number, the charges ascribed to the moving elements were all simple multiples of the charge ascribed to a monovalent atom, such as hydrogen or chlorine. All this has naturally led to the hypothesis that electricity itself is atomic. In electrolysis, at least, there is a certain minimum quantity that corresponds to a single atomic bond, and quantities of electricity transferred by electrolysis are always multiples of this unit. It was surely natural, then, to give a name to this important physical unit quantity of electricity, and it has consequently been called an "electron."

Further, in electrolysis, the electrons always appear connected with, and travelling with, certain atoms or groups of atoms. For example, in copper sulphate solutions, the positive electrons travel in pairs with the divalent copper atoms, and the negative electrons with the divalent atomic group SO_4. These charged atoms, or groups of atoms, playing such an important part in electrolysis, have been called "ions."

Now there is a very important difference between different liquids in their behaviour when we try to pass an electric current through them. Some are quite easily decomposed, others offer a very great resistance, and it has been a matter of most interesting speculation as to the cause of this. In the first place, most of the easily decomposed liquids are solutions in water, of acids, alkalis or salts, and this has naturally attracted attention. In the second place, these solutions are all ones in which double decompositions, and such-like chemical actions take place with facility. Can a common explanation be given of this remarkable coincidence of electric conductivity and chemical activity? Electric conductivity is due to two causes—first, the electric charges on the ions; and second, the independent mobility of these oppositely charged ions under electric force. Without entering upon the very interesting questions involved in innumerable speculations as to the causes of these charges and of the mobility of the ions, all modern theories acknowledge that, in some way or another, water, and some other liquids in a less degree, have the very remarkable property of conferring upon certain substances dissolved in them the wonderful independent mobility of the ions which we see in electrolysis.